

extrinsic and intrinsic—remain like, so long and no longer do their effects remain constant. What effects a change in those influences may produce experiment and observation, and these alone can determine.

For thousands of generations certain characters, such as blindness and winglessness, and enormous size of head and of jaws, and complete absence of all power of multiplication either direct or indirect, either sexual or asexual, have remained constant in "soldier" termites. No soldier termite has any marked resemblance to any one of its ancestors. Yet each is like every other "soldier," and that likeness is "heredity." Its characters have not been "transmitted," for no ancestor ever possessed them. Like conditions in successive generations lead to like results, one of which is the production of a fairly constant proportion of neuter "soldiers" and "workers" devoid of any power of reproduction. Those "like conditions" include a constant, or nearly constant, structure of the males and females, albeit the polymorphism extends to these also. The recurrence of the like conditions has been determined by natural selection, and is still maintained by natural selection. To apply the term "transmission" in such a case would be not more absurd than in any other case, supposing the word to be used in its literal sense, for, though the soldier in every case derives its characters from a line of ancestors extending back to remote periods, not one ancestor in which line has possessed those characters; yet their geographical distribution at the present time shows that the characters of "soldiers" are of remote antiquity. This constancy of characters in many generations appears to be identical with the phenomenon called "inheritance."

Heredity then is a likeness of effects due to likeness in the causes producing them. The likeness of causes has been produced and maintained by natural selection acting under fairly constant conditions. "Inheritance" is a name given to the operation of an influence which has no existence in nature. The sooner we cease to use the word altogether the better it will be for our science.

"Inheritance of acquired characters" is a mere chain of words correlated with a chain of loose ideas, but not correlated with any natural objective phenomenon. To assert it as a fact is as futile as to deny it.

C. HERBERT HURST.

Owens College, Manchester, July 26.

Echinocyamus Pusillus.

MAY I direct your attention to a rather serious error contained in the review of Ttéel's paper on *Echinocyamus pusillus*, which appeared in your number for August 3?

Your reviewer states that "not the least brilliant and far-reaching" of the advances in our knowledge of Echinoderm morphology, made in the year 1891, is the discovery by Brooks and Field of the primary bilateral symmetry "of the water-vascular system" of Asterias.

Had such a discovery been really made, it would no doubt have justified the epithet applied to it by your reviewer; but, in the first place it was not made, and in the second Metschnikoff long ago pointed out such a primary bilateral symmetry in the embryos of *Amphiura squamata*.

Field's paper, containing the results of his own and Brooks's work, appeared in the *Quarterly Journal of Microscopical Science* for 1892. In it he gives an account of the development of the larva of Asterias; but in the oldest stage which he describes there is not as yet a trace of the water-vascular system. He describes, it is true, in larvæ of a certain size a right as well as a left madreporic pore; but as all "echinologists," it is to be hoped, know by this time, the pore is primarily related to the coelom, and only secondarily enters into connection with the water-vascular system. Further, Field distinctly states that cases of a double pore had been observed previously by continental zoologists, but regarded by them as pathological; and the chief point in Field's paper is the very probable theory put forward by him that such cases constitute a distinct stage in the ontogeny of the animal.

E. W. MACBRIDE.

Zoological Laboratory, New Museums, Cambridge,

August 5.

ON referring to my draft notes, which I happen to have kept, I find in place of the words "water-vascular system," quoted by your correspondent, "pore canal system"; and I do not deny that I should have done better had I transcribed them un-

changed. I fail at the same time to see that the "error" complained of is, in its place, as serious as my critic would imply. No one who attempts to do his duty by the colossus of biological literature would fail to be familiar either with the work of the authors whom he cites, or with his own recent beginning in a kindred direction. Admitting the claims of one and all, the work of Field appeared to me to put the probability at stake upon a much firmer basis than that of his predecessors, and it was for that reason that I emphasised it. If I err not, a journalistic notice is not a thing to be hampered with names and details, especially when written with the dual object of directing attention to a really admirable monograph, and of endeavouring to promote an amicable spirit of brotherhood among workers in science, such as we to-day very much need.

THE WRITER OF THE NOTICE.

The Supposed Suicide of Rattlesnakes.

THE letter of Mr. Edward S. Holden on this subject is extremely interesting. It appears that he, like other individuals who have imagined that they have witnessed the suicide of scorpions, has fallen into the error (so common in the interpretation of biological as distinguished from abiological phenomena) of stating his inferences and beliefs as though they were observations. The "instance which occurred before my eyes" (to quote his words, which remind one of the old herbalist, Gerard) was simply that of a snake biting itself when imprisoned in a jar of water. That the blow was "deliberate," "intentional," and of "suicidal purpose" is pure speculation—and nothing occurred before Mr. Holden's eyes to warrant his entertaining such a notion. Had Mr. Holden been aware that the poison of the rattlesnake has little or no effect upon another rattlesnake, nor upon the individual from which the poison is furnished, he would probably have been less ready to conclude that the bite was one of suicidal purpose. He would then perhaps have inquired as to the depth to which the bite penetrated into the tissues of the snake, and how far such a superficial bite as a snake can inflict upon a part of its own body is likely (in the absence of any poisonous action) to be seriously injurious to the snake.

In this case, as in that of the scorpion confined in a fiery circle (experimentally studied both by myself and by Prof. Bourne, of Madras, and reported on in the Proceedings of the Linnean Society and the Royal Society) the spasmodic struggles of an animal artificially confined and tortured, have been, as we clearly demonstrated, mistaken for efforts at self-destruction. The biting of its own body by the snake may be justly compared with the "biting the dust" attributed to men who die in hand-to-hand struggle, or to the biting of their own hand or arm by unhealthy children when suffering from a paroxysm of anger.

E. RAY LANKESTER.

Oxford, August 11.

Imitation or "Instinct" by a Male Thrush?

ON the evening of July 19 a young thrush was caught in my conservatory and placed in a large outside aviary. The following morning I observed the parent birds feeding the young one through the bars with worms. In the same aviary there had been for more than ten years a male thrush which had been captured when quite young and had never been mated or troubled with family cares. On observing the parents of the young bird feeding their offspring he at once followed their example. On putting some bread and milk into the aviary he flew down, took up a piece and tried to induce the young bird to open its beak. At first the young thrush appeared to be afraid of accepting food from the foster-father, but after some persuasion it allowed itself to be fed with bread and milk, hemp seed, and other food. The parent birds were watching from the outside, and during the whole time occupied by the old male in feeding their progeny were also trying to introduce food through the bars. The day after (July 21) the parent birds did not make any further attempt to introduce food, but contented themselves by watching their young one from a tree close at hand. If any of the house-cats approached the aviary the parents would at once give the alarm. In the course of another day or two they abandoned the young one entirely to the care of the old foster-father, who has proved quite worthy of his trust, as the young bird is now able to feed himself and is in a very thriving condition. The old male still insists, how-

ever, on giving it any delicate morsel he may find. This observation appeared to me to be of sufficient interest to record in your columns, as the old male bird certainly in this case learnt how to feed the young one by observing the proceedings of the parent birds. He had never reared any young ones of his own, and had never had any opportunity of seeing other families brought up in the aviary.

E. BOSCHER.

Belle Vue, Twickenham, Aug. 1893.

Intrusive Masses of Boulder-clay.

THE letter of Messrs. Graham Officer and Lewis Balfour upon the glacial deposits of Bacchus Marsh suggests the desirability of uttering a word of caution against the assumption that boulder clay intercalated between two beds of rock is necessarily of intermediate age. I have repeatedly observed intrusions of boulder clay into the triassic sandstones of Lancashire and Cheshire, but never so striking an example as that described by Mr. Arthur R. Derryhouse in the current number of the *Glacialists' Magazine*. In his paper and the accompanying plate he shows how a series of glacial and triassic deposits were displayed in a trench in such a way as to give the impression that they were interbedded, sandstone being both below and above the glacial deposits. A minute examination established the fact that the drift deposits had been thrust in amongst the older rocks along a line of weakness due to the presence of a bed of marl. The intrusion had penetrated to a distance of *fifty yards* from the outcrop of the marl-bed.

I do not suggest that Messrs. Officer and Balfour have been misled by such an appearance, but merely warn geologists in general against falling into error.

We have heard much of late of floods and other catastrophes, even from geologists possessing a considerable intimacy with the phenomena of the British drift deposits. It would be interesting to learn in what way these injections of glacial sludge would be explained by the advocates of deluges.

PERCY F. KENDALL.

Yorkshire College, Leeds, August 14.

A Peculiar Discharge of Lightning.

I SHOULD like to add to the many recent accounts of lightning discharges the following particulars of which I have not yet seen any published account.

On the afternoon of Wednesday, July 26, during a storm at about 5.30, a blue flame was observed by some of the inhabitants of Epping to approach and shatter the chimney of a house upon the hill, occupied by Mrs. Brown and family at the time.

An examination of the interior of the house shows the discharge to have passed chiefly by the bell wires, which are fused, down one corner of a room upon the upper floor, breaking the back of a chest of drawers near, and setting the wall in the vicinity on fire.

On the ground-floor the discharge seems to have taken two paths to earth, viz. down the corner of a front room by means of some metallic damp-proof paper, and in the kitchen adjacent by means of some wooden cupboards, the doors of which were much broken and thrown across the room.

Mrs. Brown, who was seated in the front room, states that a few seconds before the house was struck she noticed what appeared to be a darkened space, surrounded by a crimson fringe of flame in the corner (perhaps a brush discharge), and her son in the kitchen at the time testifies to having seen a similar thing previous to what appeared to be the bursting of the luminous mass, which occurred with a loud report, filling the house with smoke and the usual accompanying smell of ozone. The walls are much damaged, and the polarity of a small compass in a drawer of a sideboard nearest the path of discharge was reversed. I considered the apparent forewarning of the brush discharge of sufficient interest to justify this letter.

WILLIAM BREW.

Electric Light Department, British Museum, August 8.

The Mean Density of the Earth.

IN a note in your issue of August 10, adding to the list of values for the mean density of the earth, which you gave on July 27, it is stated that Jolly and Poynting obtained the value 5.58. This is, I believe, the value obtained by von Jolly, but my final result, as published in the *Philosophical Transactions* for 1891, is 5.493

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In any account of recent work on this subject I think von Sterneck's experiments at Pribram and Freiberg deserve notice. These were made in the years 1882-5, and were pendulum experiments of the Harton Pit type. The method of comparing the times of swing of the pendulums below and at the surface was, I believe, quite new, and consisted in determining the coincidences with the same clock, which gave simultaneous half-second signals at the two stations by means of an electric circuit. The results unfortunately tend to confirm the conclusion which had, I think, been already drawn from Airy's work—that the mine method of experiment, though it may add to our knowledge of the constitution of the surface strata, is useless in determining the mean density of the earth.

Major von Sterneck's papers are published in the *Proceedings of the Militär-Geographisches Institut of Vienna*.

Penzance, Abergele, August 12.

J. H. POYNTING.

The Grouping of Stars into Constellations.

CAN you or some of your readers kindly give me an answer to the following questions, or tell me where I may obtain information on the subjects?

Did the Assyrians, Egyptians, Greeks, and Persians group the stars in the same manner into constellations? In cases where they did so were the constellations usually named by all the nations after the same animals?

How were the constellations, which we call after Greek heroes, named by Assyrians and Egyptians?

Do the different races of the present day, Chinese, Poly-nesi-ans, Hindoos, Negroes, Americans, &c., each group the stars in a peculiar way?

If each race has its own plan of grouping the stars can we make use of this peculiarity in ascertaining the affinity of various races and nations?

M. A. B.

Terriers Green, High Wycombe, August 11.

Numerous Insects Washed up by the Sea.

HAVE any of your correspondents mentioned the following fact? For the last two days, August 8 and 9, the shore at Dymchurch, Kent, and for more than two miles towards Hythe, was covered with countless quantities of winged ants washed to the shore by the waves. At low tide one sees three or four rims, so thick that each makes a black stripe, from two to three inches wide, running without interruption for more than three miles, and probably extending to a greater distance. We have had during these days winds from the north-east, very light on Tuesday morning, but strong since that.

Dymchurch, Kent, August 10. SOPHIE KROPOTKIN.

A Substitute for Ampère's Swimmer.

IN NATURE of July 27 Mr. Daniell gives a substitute for Ampère's swimmer. In Denmark we use the following simple rule given by Prof. Holten at least twenty years ago. The outstretched right hand is put in the current with the palm turned toward the magnet and the fingers in the direction of the current. Then the north-seeking pole will be moved in the direction of the thumb.

HANNA ADLER.

Copenhagen, August 3.

A Correction.

IN my paper on "The Chatham Islands: their relation to a former Southern Continent," just issued among the Supplementary Papers of the Royal Geographical Society, vol. iii., there occurs a slip in the third and fourth lines from the foot of page 9, which I should feel obliged by your kindly allowing me to correct in your columns. My attention has been called to it by Prof. Newton, of Cambridge. In quoting from his and Sir Edward Newton's observations in the appendix to Captain Oliver's voyage of Leguat, as to the "now submerged Continent," of which Rodriguez, Mauritius, Bourbon, and Madagascar are, according to them, the existing fragments, I inserted the words "named *Lemuria*, by Dr. Sclater" after the word "continent." These words of mine should have occurred within square brackets, the absence of which was, I regret, overlooked in the proof. "Now the old land-connexion," writes Prof. Newton, "of the Mascarene Islands with Madagascar, of which we spoke as probable, is not at all necessarily the same thing as '*Lemuria*,' which Mr. Sclater supposes to have reached so ne of the Malayan countries."

61, Gleebe Place, Chelsea, S.W. HENRY O. FORBES.